

of the others particulars as to the nature of the sore are wanting; in only three was it specified as a "soft" chancre. As regards the time of the first manifestation of tabetic symptoms after the syphilitic infection, the following are the facts: The symptoms of tabes developed between the

1st and 5th year in	17	cases
6th " 10th "	37	"
11th " 15th "	21	"
16th " 20th "	3	"
21st " 25th "	5	"
After the 31st	2	"
Unknown	3	"
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In order to meet the objection that syphilis occurred so frequently in the class of people under his observation that it might be considered as an accident always to be looked for, Prof. Erb gives a comparative statement of a similar examination to that of his tabetic patients, of four hundred of his adult male patients suffering from other affections, chiefly nervous, and finds that seventy-seven per cent. of these had no history of syphilis or chancre whatever, that twelve per cent. had had secondary syphilis, and eleven per cent. simply chancre. Thus in the general adult male invalid population under his observation, the tabetic cases excluded, only twenty-three per cent. were in any way syphilitic, while in the tabetics alone eighty-eight per cent. had a history of syphilis. "In fact," he says, "if one will not refuse all assistance from statistics and logic in the solution of this question, it must be admitted that these figures speak most emphatically in favor of the view that there is an etiological relation between syphilis and locomotor ataxia." Of course they are not absolutely conclusive, but they go far to support the author's views. It is well worth while for others who have large opportunities for observation in this line to make similar examinations. It cannot be said that if syphilis be proven to be at the bottom of most cases of this disease that its prognosis is necessarily improved, but it does not render it any more unfavorable, and it will be a very interesting practical point.

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HALLUCINATIONS.—Victor Kandinsky, *Archiv f. Psychiatrie*, as the result of a study on the origin and nature of hallucinations, comes to the following conclusions:

1. Hallucinations are never the expression of an aroused activity of the psychic sphere, but on the contrary are indications of the exhaustion of the same, *i. e.*, of the cortex of the anterior part of the brain. The period of intellectual delirium does not coincide with that of hallucinations. With the arousal of the psychic activity the hallucinations become less real and disappear. The ability of the patient during convalescence to engage in intellectual work contributes largely to the suppression of the hallucinations.

2. The mechanism and origin of hypnotic hallucinations are identical with those of insane hallucinations.

3. Hallucinations are distinguishable from phantasy and recollection images, however lifelike these latter may be, by their peculiar objective character.

4. Hallucinations dependent upon irritation of the nerves of the organs of sense are characterized by their simplicity; peripheral visual hallucinations frequently possess, in addition, the peculiarity of moving in series and of following the movements of the eyes.

5. The influence of recollection and of the tenor of intellectual delirium on hallucinations is extremely slight. Indeed, in delirious or insane persons the images of the fancy are not always transformed into hallucinations.

"The only difference between my view of hallucinations," says Kandinsky, "and that of Prof. Meynert is the following: According to Meynert, hallucinations depend upon the relations of the excitation of the cortex of the fore-brain to those of the infracortical centres. It is difficult for me to accept the notion that very complicated and systematic hallucination images, consisting of numerous regularly coördinated parts, and perceived by consciousness in perfected shape (for example, a landscape with water, sky, clouds, trees, houses, etc., all in their natural colors, shades, and perspective), can originate anywhere else than in the cortical cells.

The latest researches (Ferrier, Munk, and others) have shown that there are in the cortex strictly marked-out, special sensory spheres. These cortical regions are the highest centres of sense-perception, and special conceptions. Besides consciousness and abstract thought, the function of the fore-brain includes the regularities and inhibition of the excitations coming from other portions of the cortex. The power of imagery plays a great part in our mental activities, and the function of the cortex of the

fore-brain is always accompanied with activity of the cortical sensory regions (for example, the visual or auditory centres of Ferrier). If the cortical visual centre is excited, not from the fore-brain, but from a corresponding infra-cortical centre (such as the corpora quadrigemina), then the result of its activity is not a mere imaginary object, but one that assumes an objective character, *i. e.*, there is a genuine act of vision, or, in the lack of an external impression, a visual hallucination. If the control of the cortical sensory centres by the cortex of the fore-brain is prevented, then the spontaneous excitations that go to them from the corresponding infra-cortical centres (resulting from variations of the circulation or nutrition) give rise to the occurrence of hallucinations. The conditions favorable to the latter are diminution of the activity of the fore-brain, together with an excited condition of the sensory centres, cortical as well as infra-cortical. This excitation may pass from the nerves to the infra-cortical centres, hence hyperæsthesia of the sensory organs is a common accompaniment of hallucinations. But, on the other hand, we can in no case admit a centrifugal (from higher to lower centres) transmission of the excitation.

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NERVE-STRETCHING.—At the Société de Biologie, February 26th, M. P. Bert announced that he was convinced that nerve-stretching produced its effects on the cord and not on the nerve. What is it that occurs when a mixed nerve is destroyed by heat, cold, or by a caustic? Motion is first affected, while after nerve-stretching the reverse phenomenon exhibits itself. It is therefore natural to suppose that in stretching a nerve it is, in reality, the cord on which we perform the operation.

At the session of March 19th, M. Quinquaud exhibited a guinea pig, in which he had, six weeks previously, stretched the sciatic, according to M. Laborde's method, and in which the operation had been followed by trophic troubles and the spontaneous amputation of the toes to which the nerve was distributed. This is the same lesion as that produced by division of the nerve. In another guinea pig he observed a curious phenomenon to which he gave the name of *mechanical transfer*; he stretched the sciatic on one side just sufficiently to produce anæsthesia, and then repeated the operation to the same extent on the opposite side. He found then the sensibility revived and very pronounced on the